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Theory of the Seat and Causes of the Diabetes Mellitus. *By Dr.* WILLIAM ASPINWALL, *of Massachusetts.* *From his Inaugural Dissertation for the Degree of Doctor of Medicine, at the Commencement held the 27th of April,* 1808, *in the University of Pennsylvania.*

THE proximate cause of diabetes mellitus, is (in this essay) supposed to be an affection of the liver. This consists in a suspension of its secretory powers, whether arising from torpor, paralysis, or any other cause. Although dissections have not shown the liver to be particularly affected, yet how seldom is it that there is any morbid appearance after death in most diseases! nor in diabetes is it necessary. Will a paralytic limb, after death, differ in appearance from a healthy one? This opinion of the proximate cause of diabetes mellitus, as has been acknowledged, was suggested by Dr. Rush. When speaking of the liver and its functions, he says, " that it is designed to receive blood from every part of the body, in order to subject that part of it, which had not been completely animalized or divested of its chylous proper-

These proportions are not given as just, for it is almost impossible to analyze a zinc ore with perfect accuracy. We can only approximate to the truth.

It is not absolutely fair to deduce the quantity of metal a zinc ore may contain, from the weight of its precipitate by a mild alkali, as recommended by Nicholson; for this weight will vary with the quantity of carbonic acid the potash may contain, with the degree of heat to which the precipitate may be exposed, and with the quantity of water which may adhere to it.

If we attempt to analyze the ore by manufacturing it into brass, as recommended by Accum, who considers this process as *tolerably* accurate, we lose a large quantity of the metal, which escapes in the form of the flowers of zinc.

Can this ore be worked to advantage in the United States?

No information on this subject can be obtained from any book with which I am acquainted. Dr. Meade, a gentleman possessed of extensive knowledge on mineralogy, informed me, that it is never worked in England. Dr. Bruce, professor of this science, in the college of physicians, New York, told me it is reduced in Wales; and Mr. Godon, of Boston, who is extremely well acquainted with subjects relating to this business, has declared that the zinc cannot be obtained from this kind of ore in the large way, but with the utmost difficulty.

Erratum.—Page 1 of the first number of this volume, lines 7 and 8, for "in your last volume, p. 240," read "in the Medical Repository, vol. iv. hexade 2, p. 248."

ties, to a secretory process, and afterwards to pour out the product of this secretion, mixed with the liquor of the pancreas, into the duodenum, to be absorbed; or, otherwise, taken up by the lacteals, and conveyed with the chyle from the stomach, into the blood-vessels, in order to be completely converted into red blood, for the purpose of serving the various and important uses for which that fluid is intended in the human body." The product of this secretion is what the professor calls hepatic bile. It is, says Dr. Boerhaave, " mild, sweetish, and watery" to the taste. It becomes bitter only by stagnation in the gall-bladder. Does not a defect in the functions of the liver, satisfactorily account for the want of a proper assimilation of alimentary into the animal fluids spoken of by Dr. Cullen? That the liver is the viscus principally at fault in diabetes mellitus, is inferred,

1st, Because it occurs in persons intemperate in eating and drinking. The obstructions, enlargement, &c. of the liver, the effects of intemperance in drinking, are too well known to need any comment. In intemperate eaters it occurs frequently. In such cases, the digestive powers of the stomach are over-proportioned to those of the liver; for digestion cannot be supposed to be completed, until the chyle is so changed as to become a proper fluid for the nourishment of the body. The quantity of chyle must be in proportion to the quantity of aliment taken into the stomach, provided it be properly digested there. The spices which are added to give a zest to the food of all epicures, not only promote the flow of the gastric liquor, but also stimulate the mouths of the lacteals to an increased absorption. Hence the liver is excited to a secretion proportionate to the quantity of aliment received into the stomach. Hence also the increased quantity of bile in the system.

Although the liver frequently complains of the hard task imposed on her by bilious colics, sick head-achs, &c. she still continues her efforts, until, unable to relieve herself, she sinks, by degrees, under the oppressive burden, and becomes torpid and inactive. If the stomach do not sympathise, but continues to call for more aliment, and the lacteals to absorb the chyle as fast as it formed, the blood-vessels at length become surcharged with chyle. The chyle, from its quantity, becomes a stimulus to the heart and arteries, exciting morbid action in them. As the liver is incapable of performing its duty by changing it into bile, the kidneys, to relieve the system, take on increased action, which constitutes true diabetes mellitus.

2d. It is inferred that the liver is affected in this disease from the deficiency of bile, also from the costiveness and want of smell in the stools. Although Dr. Rollo believed the stomach to be the seat of the disease, he says there is reason to suppose a deficiency of bile, from whatever cause it may proceed; for, beside the light colour of the stools, there is no yellowness of the eyes, or even that high saffron-colour of the urine which takes place when the bile is sufficiently secreted, but prevented from passing into the intestines.

From what has been said as to the proximate cause of diabetes mellitus, the cure will readily be inferred. It must consist in obviating the saccharine process, in diminishing the morbid action of the stomach, and in restoring the liver to its healthy action*.

* A physician in Virginia, who has adopted the theory of the action of the liver taught by Dr. Rush in his lectures, and the doctrine founded upon it as suggested by Dr. Aspinwall, of the cause of diabetes mellitus being seated in the liver, has cured that disease by emetics, the design of which was to excite the liver, and thereby to induce that natural and healthy action in it, which is necessary to form perfect chyle. EDITOR.

Remarks on the Agency of Cold with respect to the living Body, in reply to "ARBUTHNOT." *By* A CONSTANT READER.

IN the last Medical Museum, we were presented with a few pages of "Observations on Cold," in objection to a series of experiments on the same subject, contained in the immediately preceding number of that work.

The operation of cold on the animal frame, cannot but strike every medical observer, as a subject of inquiry not only curious and deeply interesting to an inquisitive mind, but pregnant with importance in a practical sense: the former, since its mysterious effects are constantly more or less presenting themselves to our view, and the latter, because it is a very common, and often a very powerful remedy in the removal of diseases. Therefore it is with all due respect conceived, that a little dispassionate controversy on its modus operandi, will not prove an unacceptable offering to the multifarious readers of your useful Museum.

"Arbuthnot" is certainly entitled to the credit of having advocated the theory of the alleged stimulating quality in cold, with an air of prepossessing conclusion, and an irresistible spirit of ingenuity. But, at the same time, as if ingenuity was fated never to glide easily along for solid reasoning, I cannot but observe, that, after all he has done on the main point in dispute, I cannot yield my individual assent to his opinions. Perhaps complimenting his ingenuity, and rejecting his sentiments, may appear to some thoughtless readers like an inconsistency on my part, or rather like a very unusual jumble of bitter and sweet, by far too discordant for the mental palate of any common philosopher. But, in order that there shall be

no misunderstanding on this head, I shall certainly at once set about explaining my meaning. I mean that "Arbuthnot" is ingenious, not in discovering any thing really new to us, but that he is so in selecting, with more than a natural share of discernment, a few brief facts and arguments, which have long ago been advanced in support of his doctrine by Cullen, Currie, Hunter, and other writers as familiar to every student of medicine as A, B, and C to a school-boy. And when I say I cannot assent to his conclusions, I mean that I do not believe a single word of the imputed stimulating power of cold.

How it is, I shall leave for some genius more arrogant or better qualified to determine than myself; but so it happens to turn out, that my disbelief of "Arbuthnot's" doctrine is predicated on the very grounds which it appears have given rise to his conviction of its truth.

One of two things must be true: either my comprehension is dull, or his judgment is defective.

1st. The stimulating agency of cold is inferred from sensasation and motion.

Now I conclude, from the sensation and motion which usually follow the application of cold to our organs, that it is a debilitating and not a stimulating agent. And for this good reason, though sensation and motion for the most part do follow the application of cold to our bodies, yet I challenge "Arbuthnot" to prove, by a successive and lucid consideration of all the intermediate changes which occur between the impression, and what he calls its effects, that cold is possessed of an inherent power to excite either sensation or motion. By doing

this he will be richly entitled to his favourite compliment of "the homage of my highest respect."

It would have been well if he had defined to us what idea he entertained of the nature of cold. He may be a disciple of Mushenbroeck, or, for all I know, he is a believer of the more fashionable doctrine of Black. He may have become an advocate for the supposed stimulating property of cold, through an erroneous impression that it is a peculiar and independent agent of vital energy. "Arbuthnot," however, will be so condescending and obliging, as to be more explicit in his next communication.

By cold I mean that inferior temperature which is exterior to the organ affected, and I use the word inferior, in a relative or comparative sense, with the greater degree of animal heat possessed by the organ. We may say that only one of two opinions can be true: cold must singly be either a debilitating agent, or a stimulus. It either subducts a portion of the standard heat of the system, and consequently lessens excitement, or it produces no such an effect, but, after the manner and fashion of a stimulus, it infuses new and additional excitement into the part. When, however, the uncontroulable and uniform tendency of heat to an equable distribution is adverted to, the former opinion would seem correct, and the latter erroneous. Cold has no known property of affecting the animal body separate from a low degree of heat. It cannot draw the matter of heat out of the living organ, and at the same time penetrate the senses with something like Mushenbroeck's frigorific darts, to produce motion and sensation. I am aware an attempt may be made to discard such sentiments as these, by a special appeal to outward appearances and popular opinion. Ask a peasant what excites the sensation of cold when his

hand is thrust into iced-water; and being altogether ignorant of the changes which occur in the internal part of the organ, of the animal heat extracted, and of the sensation producing point of excitement which is left, ten chances to one if he does not reply, the iced-water.

But no matter; reasoning from mere appearances has often, before this, proved fatal to many a speculatist. It is like trotting after a will-with-a-wisp, which might decoy even a man of "Arbuthnot's" acuteness into a quagmire.

2d. Cold is supposed to be a stimulus because it occasions paleness of the skin, cutis anserina, and a rugous state of the scrotum.

But surely, Dr. "Arbuthnot," cold may produce all these effects without being a stimulant. By weakening the force of the circulation, a paucity of blood and fluids would necessarily accrue in the capillary vessels of the skin, thereby producing a paleness of the common integuments; and cutis anserina would be formed from the shrinking of the membranes, leaving the subcutaneous miliary glands in a projecting and visible state. And, relative to the bracing of the scrotum, it is well known to the anatomist, that elastic cellular membrane composes no inconsiderable part of that organ, and by abstracting a portion of the flaccid and relaxing influence of heat possessed by it in ordinary, cold would thereby very naturally produce a contraction or rugous state of this appendage to the genitals. In this case, however, there is no more vital action or contraction from stimulus and excitability, than there is in the bracing or contraction of a pouch of gum elastic from an exposure to a high degree of cold. But were we for a moment, to promote discussion, to admit that Albinus, Haller, and Alexander

Monro, the elder, laboured under an error in asserting that the dartos was no more than a condensation of cellular membrane, and to grant that this lining substance of the scrotum is entirely muscular as some contend, still nothing worth possessing would be obtained in support of the stimulating operation of cold.

Cold and warmth, it is well known, occasion diametrically opposite effects when applied at different times to the scrotum. While the former produces contraction in the supposed muscular fibres of the dartos, the latter occasions a relaxation in the same parts, therefore they cannot both be stimulants. Either the one or the other must be a sedative, as their modes of action are so opposite. Universal experience asserts warmth to be a stimulus: then cold must be a debilitating agent. But, in order that it shall not be said that I suffered my fondness for " the strict accuracy of logical reasoning" to get the better of my politeness, I will, out of mere compliment to " Arbuthnot," alter the proposition. We will then say that universal experience asserts cold to be a stimulus; therefore, pursuant to the premises, *warmth* must be *a sedative*.

Prithee, " Arbuthnot," what kind of philososphy is this?

Twist it about, with the implement of reason, as you please; institute one proposition, then discard it, and substitute another in its place; still the theory of the stimulating operation of cold, and the muscularity of the dartos will, notwithstanding this polite accommodation, plunge us into the greatest inconsistencies.

Well might Galen or some other ancient writer say, " ars nostra conjecturalis est," when he himself was capable of conjecturing that cold was a stimulant.

3d. The doctor's third reason in support of his doctrine, is founded on the fact, that an exposure to pretty severe cold produces shivering and muscular exertion. He asks the question as usual, can a sedative produce muscular exertions? I answer, it cannot by its own inherent agency, but it may by the aid of a well-known law of the animal economy, which " Arbuthnot" may recollect has been so ably pointed out, and so handsomely descanted on, by the author of the Zoonomia. The shivering, and the muscular exertions alluded to, are directly excited by the sensation and the pain of cold, and not by the bucket of cold water in which the feet are immersed. Or, to use the words of Dr. Darwin, " the voluntary exertions of the subcutaneous muscles are excited to relieve the pain occasioned by the torpor of the fibres exposed to cold." I am willing to grant that these causes of vital action are of a stimulant character; but my opponent must recollect that the pain and the sensation of cold are beings, or qualities, in my mind, virtually and totally different from the cold water in the bucket. The exertions of the subcutaneous muscles in shuddering from cold, are no more the primary and the sole effects of an exposure to a frigid medium, than the successive convulsive efforts of the pectoral and intercostal muscles and diaphragm, are directly and separately excited by the accidental falling of a drop of common water into the wind-pipe. In both cases, the stimulus to the muscular exertions is an unusual and highly disagreeable sensation, and neither, on the one hand, a stimulating quality of cold, nor, on the other, the drop of water.

" But a hot or a cold bath, or even immersion of the feet into hot or cold water, equally and instantly produce it." And what of all that? Suppose it does: the assertion, when granted, proves nothing to " Arbuthnot's" purpose. No; I would not grant that it proves a stimulating power in cold, if old Galen himself was to assert it. But, at the end of the paragraph, there is a kind of a maxim, or proposition, laid down, which " Arbuthnot" no doubt supposed would bear him out in this novel mode of reasoning. A hot bath, which is acknowledged to be a stimulus, and a pretty warm one too, will produce muscular exertion as well as cold. Therefore " they are both stimulants." This, to be sure, is a fine sample of " Arbuthnot's" " strict accuracy of logical reasoning." Query, which of the two is best calculated to promote the objects of science, such reasoning as this, or " the jargon of medical phraseology?" Blood-letting not unfrequently produces sleep and perspiration; therefore, with this same logical reasoning, it could easily be made to appear, that phlebotomy, " like antimoniated tartrite of potash, spr. nitr. dulc." and opium, is a decisive stimulus. The maxim alluded to is certainly, on account of its singularity and novelty, entitled to be reprinted. " When two causes produce perfectly the same effect, is it not perfectly just to conclude that both act in the same way, and, therefore, that both are stimulants, or both sedatives?" Variations from this rule are so common, that one of an opposite import has long since been proposed, and received the sanction of the scientific of every country. Instead of such a deceptive maxim, it would have been more concordant with " Arbuthnot's" seeming love of true and just reasoning, to have been guided by the following:

Opposite causes may produce perfectly the same effects, and different effects may proceed from the same causes, of vital action.

" Arbuthnot" may have found a convenience in his proposition, but it never can answer any useful end in impartial inquiry. By it, almost every sentiment or doctrine, however wild and foreign from the truth, could be established. For example, if identity of effect implies an identity of a mode of action, venesection must be powerfully stimulant, diaphoretic, and soporific, because, in different conditions of the system, it not unusually occasions an increased vigour of the pulse, sweat, and sleep.

4th. The fourth reason assigned in support of cold being a stimulating agent, is deduced from the opposite results of Drs. Currie and Stock's experiments. The former " found that the pulse of a person exposed to cold, was diminished from eighty-five to sixty-five strokes per minute, becoming firm, regular, and small." And the latter " found that it uniformly increased in frequency, and diminished in force." " Of effects so opposite, one at least," concludes " Arbuthnot," " must be a stimulant effect." It is really somewhat unintelligible to me that the experiments of Dr. Stock should be mentioned by " Arbuthnot," and supposed by him to be as fairly conducted as those of Dr. Currie, and he, therefrom, conclude cold must or " can stimulate." As Dr. Currie's report of the effects of cold on the pulse is directly contradicted by the experiments of Dr. Stock, one might suppose that " Arbuthnot" would have suspected that either the one or the other of these gentlemen must have been deceived. One proves by experiments, that cold is a stimulant, and the other equally proves by experiments, that it is a debilitating agent; and a third gentle-

man advocates only the one opinion, and at the same time supposes that both sets of experiments are correct, or fairly conducted. Why, kind "Arbuthnot," I always thought, for any thing to be a stimulant, and at the same time not to be a stimulant, according to "strict accuracy of logical reasoning," amounted to an inconsistency. By the time my ingenious adversary again presents us with an essay of "Observations on Cold," it is presumable he will make up his mind which "of these opposite effects" he considers as the indication and proof of a stimulating property. Then, and not until then, will his fourth argument admit of a more particular examination.

5th. The consideration which his third argument has received, may serve for his fifth, as they are little more than mere repetition.

6th. The sixth reason for considering cold a stimulus, is worded in the following manner: "Antimoniated tartrite of potash, spir. nitr. dulc. and warm water, taken into the stomach, excite sweating. So does a draught of cold water. Are these stimuli or sedatives?" I answer, collectively speaking, they are neither the one nor the other, but, separately considered, it strikes me that the three first articles mentioned are stimulants, and, concerning the last one, I am striving hard to merit "the homage of "Arbuthnot's" highest respect," by proving that it is a sedative. Cold water, when taken into the stomach, under certain conditions of the system, will produce increased perspiration, by relaxing the cutaneous capillaries.

7th. The efficacy of cold water in asphyxia, is deemed an additional proof of its stimulating power, more especially as "the most violent stimuli are necessary to excite action."

" Arbuthnot" here speaks of the necessity of using the most violent stimuli in asphyxia generally, and, therefore, expresses an opinion in contradiction of not only just impressions of the pathology of that morbid state of the system, but in direct opposition to the most approved and successful modes of practice of the present day. Relative to suspended animation from drowning, it is true, authors are somewhat divided. A few recommend the use of violent stimuli, but there are many, who rank high in their profession, that denounce this practice, as calculated to destroy, rather than cherish the feeble remains of vitality. But it is desirable to know the page and volume of that work which acquainted " Arbuthnot" with the efficacy of the most violent stimuli in asphyxia, from either excessive cold, or from fixed air. It is reasonable to suppose that the use of violent stimuli, in either great direct debility, as in the former case, or in an oppressed state of the system, as in the latter case, would be followed by consequences at once fatal to the patient, and the reputation of the physician. The ingenious experiments of Dr. Johnson, contained in his inaugural thesis, prove that fixed air acts, on the body, with the force of a powerful stimulus, in producing apoplexy and suspended respiration. The method of treatment should, of course, be aimed at the diminution of the undue quantum of stimuli which oppressed the system. And, to accomplish that object, I am sure, even " Arbuthnot" would " offer the homage of his highest respect," of infinitely more value than a gold medal prize, to that man who would discover better remedies than cold water, bleeding, cupping, and other debilitating agents hitherto in use. And, as it will be following a good example, to offer compliments as a premium for great and unusual discoveries, I too will offer the homage of my highest respect to that man, who will produce a single well-attested example of a revival from suspended vital action, occa-

sioned by either excessive cold, or fumes of charcoal, by the use of such powerful stimuli " as coals of fire, electricity, and many others."

8th. " A moderate degree of cold produces exhilaration; an increased degree, thirst, delirium, sleep, and death. Opium, according to its dose, produces precisely similar effects. How do they differ in their modus operandi?" I answer, contrary to what one would imagine from " Arbuthnot's" philosophical maxim, they produce precisely similar effects, by very opposite modes of operation.

9th. " Cold applications excite absorption: witness the practice of surgeons, in the treatment of ecchymosis and other effusions. Frictions, mercury, and other irritants, are also used for the same purpose. Do not all these stimulate the absorbents?"

Venesection promotes absorption: witness the practice of some of our first physicians, in the treatment of dropsy, and other effusions. Does not blood-letting stimulate the absorbents?

10th. " Cold stops hæmorrhage;" therefore " Arbuthnot" concludes it must be a stimulus. Venesection has, in a thousand instances, or more, put a stop to the most alarming hæmorrhages. All other styptics are confessed to be stimuli, why is not blood-letting?

In the estimation of " Arbuthnot," the circumstance of cold not increasing the pulse, is no well-founded objection to its being a stimulant. But this was not touching the difficulty in question. Though the fact of its not increasing the pulse

may not be incompatible with its character as a stimulus, yet the certainty which has been established by various experiments, of its directly reducing the force of the circulation, like rest, low diet, venesection, &c. completely sets at defiance all his logical acumen to prove that it is an excitant. Can my ingenious adversary mean that light, sound, and sugar cannot, when used in sufficient force, and continued a sufficient length of time, increase the strength of the circulation? If he does, he is most assuredly mistaken. But perhaps his true meaning is, that a gentle brush or scintillation of light, a moderate undulation of the air, and a few grains of sugar, applied to their appropriate senses, do not increase the pulse; nevertheless they are acknowledged to be stimulants. And, if this be a correct exposition of what he had in view, it may not be amiss to acquaint him that he might have said the same thing of opium, camphor, or alcohol, though they very justly occupy a conspicuous station among the decided excitants of the pulse.

"I take the liberty, in concluding this very hasty production, to request, that if it should be honoured with a reply, theory and speculation may not be considered as fact, and that the jargon of medical phraseology may not exclude the strict accuracy of logical reasoning."

A Catalogue of some American Minerals, which are found in different Parts of the United States. *By* ADAM SEYBERT, *M. D. member of the American Philosophical, and of the Royal Societies of Gottingen.*

Philadelphia, July 16th, 1808.

FROM the inquiries which are daily made by many of my fellow-citizens, and from the numerous applications of foreigners, who desire to have specimens of American minerals, I conceive it will not be entirely a useless task to commence a specification of the names of such as have been found in the vicinity of Philadelphia.

I do this the more readily, as the accounts we have by Gronovius, in his "*Index Supellectilis Lapideæ*," published as early as 1740, are far from being such as are to be depended upon, and as his localities are very vague, and much too general. Indeed we have reason to suppose, from the state of mineralogical science at the time he wrote, that many substances have been misnamed; for it is only within a few years that mineralogy has been cultivated with precision and advantage in Europe; with regret I mention that it has been almost totally neglected in the United States.

I conceive, that the indications which I shall furnish will convince those who are already well-informed mineralogists, that we are not so destitute of minerals as they may have been led to imagine, in consequence of no efforts of this kind having been made heretofore; and to the student of this science, I feel satisfied I shall render an important service, by placing it within his power to form a collection for himself. In this last respect, it may be a matter of some consequence, to individuals

and to our country, for if gentlemen will search to make collections for themselves, they do not simply acquire the specimen, but they, at the same time, observe its *geological* situation: thus may we ultimately be led to form a correct *geological map* of our extensive country; this only requires the united efforts of a few of our citizens, who are already competent to the task.

I do not mean to attempt any thing like a *complete description* of the minerals we will notice; this is reserved for another opportunity.

The names which will be adopted are the English of Kirwan, the French of Haüy, and the German of Werner. I think it well to give these different names, for thus will there be less risk of being misunderstood, and the information communicated may be thereby more diffused.

It is proper for me to remark, that specimens are in my collection of all the substances noticed in this catalogue.

1. *Rock crystal.* K.
Quartz hyalin. H.
Berg crystal. W.

It is often colourless and perfectly transparent. Sometimes the prism has but one *pyramidal* termination; at other times, this is the case at both ends. They vary from minute to very large, and are abundantly found in various parts of the United States. We have them in Delaware, Montgomery, Northampton, Northumberland, Berks, Lancaster, Franklin, and Tyoga counties, Pennsylvania. They are by no means uncommon in Virginia, Kentucky, and Connecticut.

Fragments of crystallized quartz, which are rolled, transparent, milky, and coloured, are found on the banks of our rivers, and on the beach bordering on the Atlantic. We have specimens of this kind, which, to say the least of them, equal, in transparency and beauty, those brought from the Brazil coast.

Very regular dodecahedral crystals, formed in consequence of the junction of two pyramids at their base, without any intervening prism (Plate XL, Fig. 1, of Haüy), are found in Franklin county, Pennsylvania. In the crystals of this locality, we have every grade of variation, as respects the form. Twin crystals are by no means uncommon.

I have one specimen of transparent crystallized quartz, which consists of an hexangular prism, terminating at each end with six minute and distinct hexagonal pyramids. It was found in Virginia.

Masses of semitransparent colourless quartz, with numerous and well defined cubic impressions, which might give rise to secondary forms, are found in Chester and Lancaster counties, Pennsylvania. Specimens, whose cavities are filled with cubic sulphuret of iron, have been found.

I found one specimen of stalactic quartz, and another of *hackley quartz* (gehackter quartz of the Germans), at the mine near the Perkiomen creek, Montgomery county, Pennsylvania.

a. Morion. K.
Quartz hyalin enfumé. H.
Rauch Topaz. W.

Single crystals and groups of them are found in Delaware county, near Chester creek. Amorphous specimens of this variety occur in Chester county, and it is found of exquisite beauty in the neighbourhood of Hanover, Lancaster county, Pennsylvania.

b. Amethyst. K.
Quartz hyalin violet. H.
Amethyst. W.

Some beautiful specimens have been found in Delaware county, near the Brandywine creek. They are in the form of single crystals, and consist of an hexangular prism, with a pyramidal termination at one end. The colour varies from a light to the dark violet. They are transparent.

I have seen some beautiful specimens, which were found in Berks county, Pennsylvania.

2. *Common quartz.* K.
Quartz hyalin amorphe. H.
Gemeiner quartz. W.

Large masses, which differ as to whiteness, transparency, texture, and hardness, lie scattered on the surface of the fields, in all directions. It is needless to specify the particular spots.

Large masses of amorphous blue quartz, which in many instances appears as the transition to calcedony, lie scattered on the surface near Red Lion square, Chester county, in the neighbourhood of the Yellow Springs, in the same county, and about Abingdon, Montgomery county, Pennsylvania.

There are specimens of amorphous milky quartz, scattered on the surface of the fields, fourteen miles west of Philadelphia, in Chester county, which may be readily separated into very thin laminæ. They are not in the least elastic.

3. *Calcedony.* K.
Quartz agathe calcedoine. H.
Chalcedon. W.

Amorphous, colourless calcedony is often thrown up by the plough, in the grounds adjacent to Trenton, New Jersey.

4. *Semi-opal.* K.
Quartz résinite commun. H.
Halb opal. W.

Considerable masses of semi-opal, of a blueish grey colour, are found imbedded in granite, at the falls of the river Delaware, near the Trenton bridge. Its transparency is much increased by immersion in water.

This mineral, of a leek green colour, and opake, lies imbedded in serpentine rock, Upper Merian township, Montgomery county, Pennsylvania.

That variety of agate, usually termed *fortification* agate, is found in Green-Briar county, Virginia.

5. *Flint.* K.
Quartz agathe pyromaque. H.
Feuerstein. W.

Large masses of common flint are found near Easton, Northampton county, and near Reading, Berks county, Pennsylvania. Rolled flints are common on the banks of many of our rivers.

6. *Hornstone.* K.
Roche cornéenne. H.
Hornstein. W.

Considerable masses of a greenish grey-coloured hornstone, with cubic sulphuret of iron imbedded in it, lie scattered on the South Mountain, five miles from the Sulphur Springs, Adams county, Pennsylvania.

7. *Woodstone.* K.
Quartz agathe xyloide. H.
Holzstein. W.

Agatized wood is found in the pine barrens, New Jersey, near Cape Henlopen, Delaware, and in Ann Arundel county, Maryland.

8. *Garnet.* K.
Grénat. H.
Granat. W.

The common garnet crystallized, in some instances, so as to present twelve rhomboidal, and in others twenty-four trapezoi-

dal planes, is found imbedded in *micaceous shistus*, eight miles north of Philadelphia, east of Germantown, and also west of the Wissahicon creek; in *granite* on the banks of the Schuylkill; and in *granite*, near the town of Chester, Delaware county, Pennsylvania. They are found from very minute to such as weigh but little less than four ounces. The same are found near Chesterfield, Massachusetts, and near Bolton, Connecticut.

Rolled garnets are common on the borders of the river Schuylkill, four miles north-west of Philadelphia.

Very handsome black *trapezoidal garnets* are found in the *granite* which abounds near Germantown.

I have a single specimen of Haüy's *grenat émarginé*, from Albermarle county, Virginia.

9. *Zeolite.* K.
Mézotype. H.
Zeolith. W.

Radiated *zeolite* is found investing hornblende rock, on the canal near the river Schuylkill, about three miles and a half from Philadelphia.

10. *Beryl.* K.
Emeraude. H.
Beryll. W.

This mineral is found in the *granite* of Chesnut-Hill, and in that brought from Ridley creek to this city, for the purpose of building cellar walls, &c. I have received a specimen of

this mineral from Maryland, which far surpasses the former in hardness: it much resembles some specimens from Brazil.

In all the above instances the *beryl* is crystallized in the form of hexangular prisms.

11. *Tourmalin.* K.
Tourmaline. H.
Turmalin. W.

Both the shorl and tourmalin of authors are found abundantly in the neighbourhood of Philadelphia; some of which are of considerable size and beauty. They occur in *granite* at Sheridan's ferry, on the Schuylkill; in that of Germantown and Chesnut-Hill; also in that near Ridley creek, Delaware county, and in Chester county, Pennsylvania. Some elegant specimens have been found, in amorphous quartz, in Delaware county, Pennsylvania; near Elkton, Maryland; and near Boston, Massachusetts.

12. *Hornblende.* K.
Amphibole. H.
Hornblende. W.

Different varieties of this mineral abound in the United States. We have the *indeterminate* in Philadelphia, Delaware, Chester, and Lancaster counties, Pennsylvania; in Morris county, New Jersey; and near Troy, in the state of New York.

Considerable strata of the *fibrous* occur near the falls of the Schuylkill, five miles north of Philadelphia.

To be continued.

A Case of Tetanus, successfully treated with Mercury and Cold Water. *By* Joseph Klapp, *M. D.*

Preliminary Remarks on combining Theory with the Practice of Medicine.

I HAVE sometimes heard gentlemen, both theorists and practicalists, undervalue the worth of medical publications, because they contained more theory, or relation of practice, then was calculated to please their respective and opposite turns of mind. And, in a few instances, I think, I have perceived something like envy manifested, and slander used to depreciate each other's method of cultivating medicine, and thereby to stamp on their own peculiar course of inquiry a stolen air of importance, and a display of orthodoxy, not in reality their own.

The imputations of he is a fanciful theorist, and he is a dull or dogmatic practicalist, are, now-a-days, by far too common.

Such a fashion as this must retard the cultivation of science, and, if persevered in any great length of time, may bind its devotees in the fetters of prejudice and ignorance. Theory, without practice or experience, is a floating bubble, which, with inflated grandeur, may serve to catch the eye, or the fancy, until its momentary form or existence bursts into airy nothing; and practice, without the guide of science, is downright jargon or empiricism. The anticipated temple of medicine, which is to constitute the ultimatum and the pride of our art, cannot be built until the nail of physic be properly driven, and

the permanency of the work must much depend on the quality of its materials. Useful knowledge can only grow out of a judicious combination of the labours of the theorist and practitioner.

The epicurean could not relish the favourites of his palate without they were flavoured with pepper and salt; and the mental epicurean, who would partake of the practice of medicine without the pepper and salt of theory, must be possessed of internal senses too rude and easily satisfied to be capable of any great expansion of intellect. Therefore, to make the banquet both substantial and grateful, let the practice be skilfully peppered and salted with the theory of medicine.

THE subject of the following communication is the relation of a case of Tetanus.

On the 10th of May, I was requested to visit a boy in Penn-street, who was much indisposed with what his father called a convulsive affection. The disease was found to partake of that character to an alarming degree. It was a confirmed case of tetanus, accompanied with alternate rigid contraction and partial relaxation of the temporalis and masseter muscles, presenting something of an example of a clonic trismus.

The history which I obtained from the family of the imputed cause and the primary advances of the disease, will initiate the reader into a more perfect understanding of its true character, than can well be derived from any account which I could give of the aspect and symptoms of it, when I first saw the pa-

tient. For some weeks prior to his being taken seriously unwell, he had been troubled with an obstinate phagedenic ulcer on one of his feet. It had formed without any visible cause which could be assigned, and resisted all the common plasters and unguents which are generally used on such occasions. At length an ointment of a particular composition was recommended by a neighbour, and applied with the desired effect of restraining the discharge, and closing the ulcer. The boy, however, was not thereby benefited, for symptoms soon occurred which manifested that the local affection of the foot was exchanged for a dangerous systematic disease. In a short time, something of a stiffness and uneasiness was complained of about the jaws, with an increasing inability to masticate his food; and, almost coeval with this affection about the jaws, a considerable degree of stiffness was induced in the muscles of different parts of the body, but particularly in those of the lower extremity on the left side. As often as four or five times during the day, and more frequently during the night, a violent pain in the scrobiculus cordis would occur, and which, in every instance, was a certain precursor of a paroxysm of spasmodic contractions in the muscles of the neck, trunk, and extremities. During the intervals of these paroxysms, the lad would seem pretty well, and, being naturally of a quiet and contented disposition, he would hobble about the house without making any particular complaint. In this situation, but sensibly growing worse, he had been for four or five days previous to my seeing him.

Having made up my mind concerning the character of the disease, I next began to deliberate on the course of treatment which should be pursued. Reasoning, a priori, on the particular nature of the disease, from what I had met with in practice, and from published accounts of the success of different reme-

dies, I was sensible of a decided prepossession for a salivation, over every other remedy yet proposed. The short protest of Dr. Moseley against the practice was not forgotten, but the authority in its favour was deemed at once so respectable and decided, as to induce me to venture on its use. He notices a salivation in this disease in the following words: "If it be urged that the application of mercurial frictions is an invention of the moderns in this disease, I answer, it is my opinion that mercury used in the tetanus *has killed more people than it has cured.* And further, that I suspect those who have recovered when this remedy has been used, would have *recovered without it;* for many people have been attacked by the tetanus, in the West Indies, under a course of mercury."

The doctor's experience with this remedy certainly differs in no small degree from that of several eminent physicians, both in this and other countries. Perhaps climate and some peculiarity of circumstances may account for this great variance in medical experience. But when the abruptness and apparent warmth with which Dr. Moseley expresses himself on this subject is adverted to, one might feel himself half inclined to suspect, that the use of mercury in tetanus has been opposed on grounds not altogether scientific and dispassionate. He certainly passes over its consideration in a manner more brief and positive, than a remedy of such reputed efficacy and powers, in the most of diseases, would seem to deserve. But it must be recollected that Dr. Moseley is a practical man, and therefore may be entitled to speak with such conclusion and certainty.

Agreeably to the course of treatment which appeared to me most conducive to the recovery of this particular case of tetanus at any rate, I began with a mercurial purge, to obviate a confined state of the bowels, and produce a tendency in the

system to the meditated ptyalism. After the alimentary canal had been freely opened, nine ounces of blood was taken from the arm, to lessen the force of the pulse. To change the condition of that spot which was the alleged inlet of the disease into the system, caustic was very plentifully applied, and the foot enveloped in an emollient poultice. The next day, on the 11th, when I renewed my visit, the disease was found pretty much in the same state as the day before. The application to the foot, on the spot which had been the seat of the ulcer, had produced some inflammation and corrosion of the flesh. The pulse still continued active, and the loss of nine ounces of blood was again ordered. Two grains of calomel were prescribed to be taken, in the form of pills, three times a day. The caustic was renewed, and also the poultices. The spells of convulsions were this day more alarming.

12th. The disease was still progressing, and the paroxysms of longer duration and more frequent recurrence. This day, during a contest between the muscles on the anterior and posterior parts of the trunk, and the flexor and extensor ones of the extremities, the patient was completely thrown out of bed; and, for the compass of about half a minute, his body presented a strong example of that form of tetanus called by authors opisthotonus. His breast and neck were forcibly thrust forwards, and his head and arms, with his hands firmly clinched, were drawn backwards. On the first invasion of the disease, his countenance lost its natural figure, but on the accession of the convulsions it was very commonly distorted into a risus sardonicus. After the second bleeding, the pulse subsided full as much as was deemed consistent with the welfare of the patient. From being rather full and active, it became small and frequent. The jaws were closely locked during the paroxysms, but, in the intervals of the convulsions, they could be

opened a little, so as to admit into the mouth drinks and medicines.

As the caustic did not produce that discharge from the foot, or the degree of inflammation which were conceived necessary, I placed a blister over that part where it had been applied. Calomel pills to be continued, and three ounces of ung. hydrarg. fort. to be rubbed on the arm-pits and shoulders, in twenty-four hours. To promote the diffusion of mercury through the system, a coarse linen band, with ung. hydrarg. fort. spread on its internal surface, was ordered to be constantly worn about the jaws, in such a way as to pass up over the ears, and fasten on the top of the head. It appears to me that a mercurial linen, or soft leather band of this description, is a contrivance which is entitled to future attention from practitioners. The air inhaled through the mouth of the patient will by it be impregnated with the vapours of the mercury, and the system must thereby be speedily charged with the medicine. In this way it will doubtless materially forward a salivation.

13th. No better; the paroxysms very alarming, and attendants seem convinced that his misery cannot be protracted much longer. The intervals of relief from the convulsions are of less continuance than at any previous period. The pain in the region of the stomach appears almost insupportable. During my visits of to day, he was very severely handled with two or three paroxysms of universal convulsions, and I availed myself of this opportunity to make observations on those changes of his countenance and pulse, which preceded and followed the convulsions. A paleness of the face, a lividness of the lips, a thread-like pulse, and a violent pain in the scrobiculus cordis, were in every instance the common precursors of the paroxysms; and, on the cessation of the convulsions, the counte-

nance resumed something of its colour; a period of relaxation, amounting almost to fainting, with a great degree of flaccidity in the flesh, ensued. In this condition of the system, although muscular strength appeared nearly exhausted, I constantly found a degree of force and fulness in the pulse, which did not occur in any other state of the disease.

These premonitory and succeeding symptoms of the paroxysms, would seem to favour that theory of authors, which supposes a libration or fluctuation of excitement between the muscular and vascular systems, to constitute the nature of tetanus. In all probability, tetanus, and every other disease incident to the animal body, consists of partial, and not of general morbid excitement. The great Brown paved the way to an important discovery in pathology, when he drew the attention of readers to the elevation and sinking of excitement in diseases; but, like many other original geniuses, and liberal improvers of the medic art, he travelled far with bold and rapid strides on the right road, yet, by some treacherous guide of ideas, he was doomed to be led astray just as he was treading the threshold of truth. In every disease there is certainly a sinking or elevation of excitement, but this departure from healthy action does not appear to consist of that change in the general quantum of vital action which Dr. Brown defines it to be. Speaking of the living system collectively, perhaps daily experience can furnish impressive examples of diseases in which the degree of excitement is neither more nor less than that of the most perfect health. In health, the movements of the animal frame are in concert and harmony with each other, but in disease, they are crooked and irregular.

The blister produced a pretty copious discharge from the foot, and the frictions with the mercurial ointment were directed to be continued.

14th. The boy was this day sensibly worse in every respect. The medicines were still persevered in, with the addition of an anodyne draught at night.

15th. The foot was dressed with an ointment of cantharides. Mustard poultices were applied to the wrists, and a blistering plaster on the region of the stomach. There were scarcely ten minutes together, this day, when it could be said he was clear of spasms or convulsions in some part or other of his body. To the inventory of his sufferings was added a strabismus, and an occasional difficulty of breathing. On some occasions I thought something like delirium could be discerned; but on this point I am not quite certain. I may have been deceived. At length a ptyalism was announced by a fluxion of saliva, and a fœtor of the breath. The mercurial remedies were discontinued.

16th. The mouth was becoming very sore; but the salivary discharge was not as copious as might have been expected. The disease did not appear to have yielded in the least.

17th. The situation of the patient seemed much the same as the day before, and, on leaving him on the afternoon of this day, I acquainted his parents, that, in my opinion, it was extremely doubtful whether he would live until next morning.

18th. I was agreeably disappointed in finding him rather better. The convulsions were less frequent and less severe. His face presented a scarlet complexion, from the tumefaction

of his cheeks, and the inflammation of the mouth. In the evening I found the tetanus still retreating before the powers of a salivation.

19th. By invitation, Dr. Rush was so obliging as to visit the patient with me this day, and the result of our conference was a determination to associate with the salivation the use of cold water. We were led to this remedy by the reputation which it has obtained in the cure of tetanus, and by the existence of those particular symptoms in the case, which are considered very favourable to its use. Linen sheets were dipped in buckets of pump water, whose coldness was increased by mixing with it culinary salt. These sheets were applied so as to envelope the whole body of the patient. The boy was wrapped up in this manner for about one quarter of an hour: the sheets were then removed, and, after wiping him quite dry with some soft flannel, he was again removed to his bed. The first time this remedy was used, the lad was sensibly benefited. It evacuated the febrile heat from the surface of the body, and produced a very general and a very refreshing perspiration. The sheets were repeated twice this day, and with equal utility. The remedy operated like an anodyne, in composing the agitation of the muscular system, and producing kindly sleep, which had not before been experienced for some time. Thirty drops of liquid laudanum were ordered to be given in the evening, and, to allay a troublesome spasm which now and then fell on the bowels, an opiate enema was exhibited.

20th. Continues better. Has had only two slight paroxysms of convulsions this morning. The salivary fluxion from the mouth is very profuse, and the violence of the inflammation occasioned by the mercury, and the tumefaction about the jaws and face, seem considerably abated. The inflammation first

produced in the mouth appeared to exceed that particular degree at which a free discharge of spittle takes place, and the cold water, which was applied with napkins, and in the form of lavation to the face, had the effect of reducing it down to a point of excitement which was favourable to secretion. His naked body was wrapped in the sheets of cold water four times this day, and in every instance gave such decided relief as to dissipate from the mind of the patient, though a child, all antipathy or dread. The anodyne enema proved successful. Thirty drops of liquid laudanum to be given on returning the patient into bed, after the last application of the water this evening.

21st. Has had no return of the convulsions since yesterday morning. Seems much composed, and, comparative with the last few days, is pretty free from pain and uneasiness. The bowels being somewhat confined, a laxative glyster was ordered. The water was applied the same as yesterday. Since the patient's case has taken this favourable change, the pulse has become regular, fuller, and, of course, less frequent.

22d. This day the mercurial fever seems completely triumphant over the tetanic affection. There has been no return of the convulsions, and the jaws appear to labour under no restriction or difficulty but what may be imputed to the salivation. The violent pain in the scrobiculus cordis has disappeared with the convulsions, and the patient's sensations generally are much altered. The pulse is somewhat accelerated, and the swelling of the face is this day increased. The fluxion, however, of saliva tinged with blood still continues profuse. To abate the heat in the mouth, and the tumefaction in the face, napkins of cold water were ordered to be constantly kept about the jaws. The more general application of cold water was continued as before, and, provided the febrile heat of the skin and the un-

easiness of the patient are removed or lessened by the last application of it this evening, thirty drops of laudanum to be taken.

23d. I was this day convinced that the state of my patient's system had experienced a very considerable change for the better. Excepting an occasional spasm of a moment's duration, in different parts of the body, he does not appear to labour any longer under either tetanus or locked jaw. Though the ulceration of the mouth is considerable, and though his face still continues much swelled, yet he can now open his jaws and speak pretty well.

The napkins of cold water, which were directed to be applied to the face, had the desired effect of lessening the heat and uneasiness of the mouth and throat; and, in addition to which, the swelling and hardness of the cheeks were thereby much abated. As the remedy was comfortable and desirable to the patient, it was ordered to be liberally continued, and also to allow him to wash his mouth often with cold water. To open the bowels, senna and manna were ordered. The evening anodyne was continued.

As his only complaint, at present, appears to be that which was intentionally excited by me, the further application of the sheets of cold water was discontinued. The velocity of the pulse, and the uncomfortable warmth of his body, are less than yesterday. This morning he asked for food, but the state of his mouth would not allow of much indulgence to his appetite. To preserve in due subjection the fever from the mercury, which was much disposed to rise into an injurious excess, laxative medicine was given, and twenty drops of spir. nitr. dulc. and antimonial wine, every hour, to produce a cool-

ing and febrifuge perspiration. Alum water was used as a mouth wash.

24th. The state of the patient this day, was what might reasonably be looked for in a progressing recovery. The ulceration and inflammation in the mouth are diminished, but the fœtor of his breath, and the fluxion, continue very considerable. No disposition to spasms or convulsions has returned. The faculty of speaking, and mastication, is improving. His pulse is somewhat active to day, but I could not perceive any other visible indication of fever. His skin felt cool, and was softened with a mild perspiration. The laxative medicine made him more comfortable, and less febrile. The diaphoretic drops were continued at longer intervals.

25th. His recovery is happily advancing. Every symptom of both opisthotonus and trismus still is absent; mouth is better; appetite rather excessive; has been pretty freely gratified. For the last night or two, the evening anodyne has rather increased his uneasiness, than afforded the desired rest; therefore ordered to be omitted. Astringent wash for the mouth to be continued.

26th, 27th, and 28th. In a state of advancing recovery; mouth better, and no signs of a return of tetanus. Appetite good; and what is equally conducive to a reinstatement of strength and health, is a good faculty of masticating his food.

As a further history of this case would consist of nothing more than an uninteresting account of the gradual subsidence of a salivation, and as such a detail must be unnecessary, it will now be terminated.

It may, however, be proper to add, that several weeks have elapsed since my attendance on this patient was discontinued, and that, at the present time, he has nearly recovered his ordinary strength and health.

OBSERVATIONS.

In the case which I have just related, there are two facts to which I wish more particularly to call the attention of physicians:

1st. The efficacy of a salivation in tetanus.

2d. The safety of using mercury and cold water at the same time.

Southwark, June, 1808.

An Account of a Worm discharged by the Urethra. By J. STUART, *M. D. &c.*

CONSIDERED a priori, and abstracted from all observations and records on the subject, the existence of animalculæ in the urinary bladder would appear highly improbable, if not absolutely impossible. No animal with which we are yet acquainted, can live without the all-vivifying principles contained in atmospheric air; and so recluse is the cavity of the bladder from external communication, that we may justly doubt whether its contents are ever visited by this benign pabulum of nature, in a disengaged state or not. An obstacle of no less difficulty is also presented in the extreme acrimony of the fluid which is constantly poured into this sack. But, should we

turn our eyes a moment to analogical facts, should we consider that animals, apparently, of much greater importance in the scale of animated nature than worms, have been discovered in the centre of large and solid masses of pit-coal, at the distance of many fathoms beneath the surface of the earth, in the middle of the wood of living trees, and even in the heart of solid rocks, whatever difficulty we may find in determining the source from whence they may derive their existence or support, we shall not find the least obstacle to concluding that animalculæ may meet with a congenial habitation in every part of the human frame.

Nor will conclusions drawn a priori meet with less opposition, when we consider animalculæ in abundance are found in various fluids of much greater acrimony than that of the urine of man. Millions of these, of the most delicate structure, are seen sporting away an ephemeral, though a frolicsome and apparently a happy existence, amidst an aggregation of the acrid and corrosive particles of the acetous acid. The stagnant and putrid waters of the lake want not inhabitants, whilst living beings innumerous, of all forms, sizes, and descriptions, glide in safety through the waves of the briny deep. Nor stop we here in support of the doctrine advocated. Should we appeal to direct facts, what a severe reproof will god-like reason meet, when, from the best authority, we shall find the knife of the anatomist has never penetrated that secret recess in the mazy fabric of organic life, which has not, at some time or other, been demonstrated the abode of animalcular existence.

Baglivi tells us of a worm found in the *pericardium;* Senac of one in the ventricle of the heart; and Du Verney of one in the brain; while Aduanus Spigilius gives testimony of one dis-

covered even in the *vitreous humour* of the eye itself; and to these, were additional facts required, hundreds of observations might be added to the same effect*.

But considering it unnecessary to detain the reader any longer with facts he must frequently have met with before, I shall willingly dispense with all further quotations, to proceed to the subject of my own observations, in a case of a worm discharged from the URETHRA.

T. B——, aged 12 years, on the 30th day of May, 1805, was attacked with the usual symptoms of pyrexia. Soon after the establishment of fever, he was attacked with an acute pain in the left side of the thorax, which was accompanied with a severe and troublesome cough. His face was flushed; his eyes watery and inflamed; his tongue was white, and exhibited the general appearances observed in inflammatory fever; the breathing was oppressed and laborious; the skin was dry, and the heat intense; the pulse was active, chorded, and strong; his intestines were costive; complained of lancinating pains in the abdominal region, while his stomach was now and then disordered with a deathly sickness, without any disposition or effort to vomit. On being raised up in an erect posture, he never failed to evidence the greatest prostration of strength.

These symptoms evidently indicated an affection of the pleura, combined with an increased determination to the abdominal viscera; while the indications of cure were so striking and obvious, that were there not room to suspect the remedies employed, of inducing some of the most prominent symptoms

* See American Philosophical Transactions, Vol. 2, for an account of a worm in a horse's eye. EDITOR.

in the course of the disease, which were afterwards proved to depend upon the exciting cause, the treatment might, with the greatest propriety, be passed over in silence.

Bleeding, refrigerating and stomachic medicines, such as spiritus mindereri, with diluent drinks, were first prescribed, and the former frequently repeated until the 4th of June, without the relief generally expected under similar circumstances of disease. The intestines being rather torpid, and the anorrhexia ceasing, a dose of calomel and jalap was prescribed with some relief. The pain, however, notwithstanding the frequent bleedings, still continued with little palliation, while the fever raged at intervals with nearly the same violence as on the first aggress of the disease, until the morning of the 4th, when the same purgative medicine was repeated, and at night an epispastic applied to the side affected. On the morning of the 5th, the symptoms were much alleviated, but the patient complained of a dysuria, which almost amounted to a total suppression.

This part of his complaint being attributed to the operation of the cantharides on the urinary organs, demulcent drinks, with occasional doses of spir. nitr. dulc. were prescribed to be taken internally, and the region of the bladder to be frequently bathed with camphorated ointment. On the morning of the 6th, I was much gratified to find my patient had passed a salutary crisis about seven o'clock of the evening before, while, to my great surprize, I was presented with a glass urinal containing the WORM, from which was taken the annexed engraving. I was informed by the nurse, this insect was expelled suddenly, with the usual stream of urine, from the URETHRA; that thereon the suppression, and all the symptoms of disease before present, suddenly disappeared.

Although, in the course of my reading, I had often met with cases of worms discharged from the urethra, yet, under my own observation, a case of this kind, attended with symptoms of pleurisy, was singularly novel; whilst a recollection that the existence of worms in the urinary bladder had been much contested, proved an additional stimulus to make every inquiry calculated to satisfy the curious, and to dispel such doubts as scepticism might affect to raise. As the contents of the bladder had been received immediately from the urethra, in an urinal of the ordinary construction, there was not the least reason to suppose the worm could have been introduced from an external source, while the suppression of urine preceding the copious discharge immediately following the appearance of this little animal in the urinal, fixes its origin beyond all possibility of doubt in the bladder of urine, or the kidneys.

It has been pretty generally received, and probably in some instances well ascertained, that those bodies passed by the urethra, and taken for worms, have, upon further and more accurate examination, proved nothing more than a quantity of coagulable lymph, effused by excessive action into the kidneys, and by the ureters moulded into that form under which they were discharged. But these could never have taken on motion and other characteristics peculiar to animated beings, much less the form exhibited in the annexed engraving; whilst the subject of these observations afforded phemomena of life and motion for several hours after it was ushered to the light, and, as is here seen, presented to the eye of the spectator very perfectly the form of organic matter. It partook of a pale flesh-colour, and, at the first view, very much resembled the teres commonly found in the human intestines. More close examination, however, showed it to be firmer and more elastic;

the form was more slender and delicate, while the skin wanted that transparency which in these gives the eye an opportunity of scrutinizing the various convolutions of the intestinal canal, and the intricate and very extraordinary structure of the spermatic vessels*. When subjected to the prying powers of the microscope, it exhibited more of the general outlines of those worms found in the intestines of calves, and so accurately described by the learned and ingenious author just cited. One end terminated, more abrupt than the other, in three spherical tuberosities, the surface of which afforded to the eye, assisted by the microscope, a fungous and somewhat granulated appearance. These, in that point of their peripheries most remote from the common centre, described a triangular figure, whilst in this centre was seen an orifice of the same form, which is conceived to be the beginning of the œsophagus or mouth. Of these tuberosities, one was nearly as large as the other two, and seemed to form a counterpoise to them. On the external periphery of this, and near the points which came in contact with the other two, were discovered two extremely minute and shining black spots, which, from their position, and from the uniformity and regularity of their appearance, were supposed to be eyes. In this observation, notwithstanding the assertion of the learned and ingenious Vallisnerus, I do not stand alone; Le Clerc, Redi, and many others of equal eminence and acumen, all concur in having observed, in the Teres both of men and calves, what they supposed to be eyes.

The other extremity terminated in a hook-like point, which, with the back of the worm towards the spectator, seemed a regular continuation of the body; but viewed in profile had more resemblance to the nail or claw on the end of the toes

* Vide Redi.

of the Gallinacea. At the origin of this projection, and between it and the protuberance answering to the end of the toe, were seen several rugæ, radiated and diverging from a common centre, which last proved to be the origin of a second cavity leading into the body, and which may be supposed to be the anus. Many inquisitive and learned naturalists concur in having observed in the teres a third orifice. They inform us this is situated in the under part or belly, at the distance of about one-third of the length of the insect from its head; and is so patulous, as when once descried by the microscope, to be easily perceived by the naked eye; and this, it is conjectured, is appropriated by nature to the exclusive purposes of love and the propagation of the species. Of the accuracy and correctness of the observations of such naturalists, very little doubt can be entertained; but whether it should be attributed to the minuteness of the subject of my observations, to its imperfect formation, or to the too weak powers of a powerful microscope, this orifice has effectually eluded every attempt of mine for its discovery*.

The circumstance in this case most calculated to arrest the attention of the practising physician, is the assemblage of symptoms preceding the expulsion of the worm. Irritation upon different organs, from the various existing sympathies between these and other parts of the same system, are known to exhibit a train of symptoms of a peculiar character, according to the organs or parts primarily acted on. A puncture under the nail produces pain and swelling of the lymphatic glands in the axilla of the same side; worms crawling in the *stomach* produce an itching of the nostrils, dilatation of the pupil, and frequently vertigo and syncope. Riverius, Bianchi, Schenckius,

* It is believed that the orifice above alluded to, is only seen during the season of the amours of these classes of animals. E.

de Meyserey, and Marteau all bear testimony they have seen a pleurisy produced by the same cause; while the learned Sauvages tells us he has often witnessed the same occurrence. "*Frequens*," says he, "est hæc *pluritis* apud *pueros;* dolor est lateris punctorius, sæpe ad costas infimas, cum tussicula sicca, quandoque suffocativa, ut in amphemerinâ quintâ dictâ (vel coqueluche); pulsus durus, respiratio difficilis, quandoque singultus, genæ nunc calent et rubent, nunc frigent et pallent. Hic morbus frequens mihi occurit et sanatur, sine phlebotomia saltem repetita, leni emitico, catharticis, et vermifugis, non neglectis bechichis."

But that symptoms of inflammation in the pulmonary organs should occur from any irritating cause in the *kidneys*, *ureters*, or *bladder*, can scarcely be accounted for by any acknowledged sympathy in the animal economy. It is true Aretæus, in his description of the symptoms arising from the descent of a stone from the kidneys, has made some remarks which may lead to the belief that he had met with pleuritic symptoms from such irritation. "Quod se aliquando grandior effectus," says he, "lapis concavo renis inhæserit, hunc dolores lumborum, circa musculos quos psoas appellant, excitantur, et ad medias usque costas perveniunt. *Multis certe dolor ille, ac si pleuritis esset, imposuit:* preterea gravitas adest coxendicis: autem in ureterem lapis inciderit, concussio corporis fit, ut in rigore: calculi progressus sentitur cum violenta contentione." And Baglivi has recorded a case to the same purport.

A young man, of twenty-eight years of age, had for ten months laboured under a difficulty of breathing and pain in the thorax, which were occasionally alternated with paroxysms of vomiting, and a sense of weight in the abdomen. After various remedies had been tried in vain, he at length expired.

Manifest causes of the complaints the poor sufferer had endured were observed; but, besides these, others were discovered which never had been suspected to exist, and which no ways corresponded with the symptoms of his complaints: for his urine had never deposited any nephritic gravel; he had never complained of any affection of the kidneys; nor had ever suffered a suppression of urine. Yet the right kidney was very remarkably enlarged, and of a hardness that approached that of cartilage, insomuch "that it was with great difficulty it could be divided with the knife, and contained a stone that weighed six ounces, &c. The whole substance of the left kidney had disappeared, and the cells thereof were filled with a greenish fluid." But since in the case, fever, cough, and the various symptoms which constitute pleuritic affection, have not been enumerated by this accurate observer, we may reasonably conclude that the pulmonary symptoms were spasmodic, and consequently of short duration only. And, indeed, should his phraseology be examined with an eye of criticism, it will evidently give the strongest support to, if not absolutely confirm this position: Multis certe *dolor ille*, ac si pleuritis esset *imposuit*. By this it will be seen it was not the case of the patient; it was not the general assemblage of symptoms which imposed on many *as if it were* a pleurisy, but *ille dolor*, that pain, or that particular symptom only, *qui multis imposuit*. As the affection of the breast, in the last case, appears to have been of many months' standing, we may fairly infer it was chronic, while the dissection evidently proves the stone in the kidney was rather an accidental concomitant than the cause of his complaint. Hence, these and such other cases being rejected, it will be seen there is no example of pleuritic affection produced by immediate sympathy between the legitimate seat of the disease and the urinary organs; consequently, unless we should throw ourselves upon the high-sounding word Idiosyn-

crasia, as a reposing place for our ignorance, we shall be compelled to seek some other means for the explanation of the phenomena under consideration. These, it is conceived, will be found eminently conspicuous in PREDISPOSITION and IRRITATION.

That certain causes, acting on the surrounding medium in which we live, so operate as to induce a predisposition to particular diseases at particular seasons, rather than at others, cannot be denied. Hence, in the vernal months, inflammatory affections are most prevalent; whilst at others intermittent, remittent, typhus, and bilious fevers open the most patulent and common outlets to the life of man.

Nor is it less certain that fever occurring from irritation always assumes a character corresponding with the particular diathesis prevalent in the system, at the time when the irritating cause is applied. In confirmation of this, various topical applications, such as blisters, and even leeches, applied to external parts at a time when the general system is under great prostration from the excess of stimulus, are frequently followed by gangrenous fever; and I have lately seen a case, in which a general gangrenous action of the whole sytem was induced, by the irritation occasioned by cutting the gums of a child, to mitigate the symptoms of dentition. It is also well known that fever once formed, of whatever character it may be, is more disposed to produce topical affection on some particular part of the system, in proportion as that part is under greater debility or excitability, or, in other words, as it is under a greater predisposition to suffer from the commotion excited. In confirmation of this position, almost all inflammatory affections are preceded by pyrexia and general disorder of the whole system, before the patient is sensible of the least topical dis-

ease. Hence, also, a person having been several times affected by catarrh, is ever afterwards, on exposure, more obnoxious to it than he who has never before been affected by that complaint.

From the history of the case, and from the principles here advanced, it appears that the irritation produced by the worm on the urinary organs, acted first by inducing general fever, and that the topical affection was the consequence of the fever determined to a part previously affected by a peculiar susceptibility, or predisposition to disease. Thus, when pleurisies are endemic, any exciting cause inducing fever is almost constantly succeeded by topical inflammation in the seat of that disease.

DESCRIPTION OF THE PLATE.

Fig. 1. Exhibits the worm in its natural size.

a. The head.

b. The tail.

Fig. 2. Exhibits the same as it appeared under the microscope; which figure, for the accommodation of the plate to the size of the volume, has been divided into two parts.

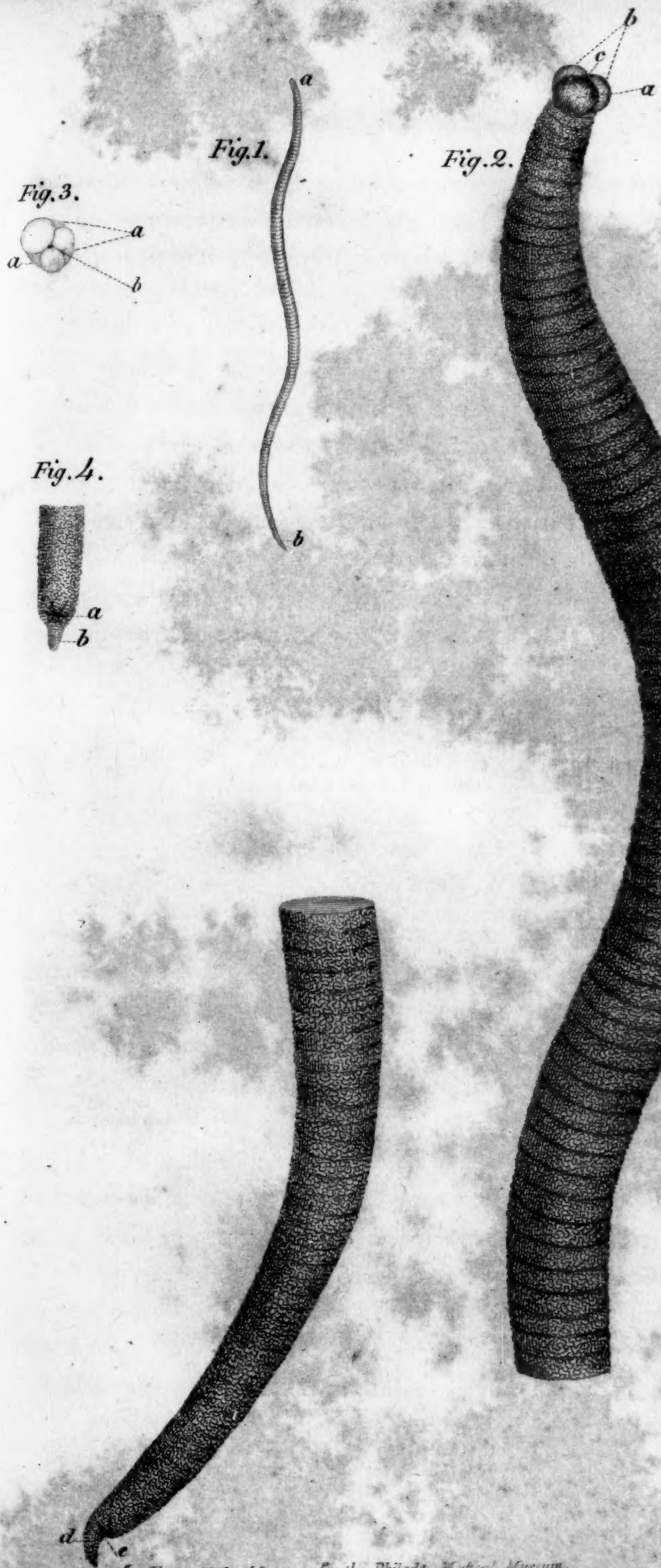
a. b. c. Represent the head, which was composed of globular tubercles; one larger, and two smaller ones.

a. The larger.

b. The two smaller.

c. A triangular cavity, formed by the approximation of the two protuberances before mentioned, in which could be plainly observed a considerable orifice, supposed to be the mouth.

d. e. Represent the tail.



Stuart M.D. del. Engravd by A.Lawson for the Philada Medical Museum.

d. The profile of the curious hook-like projection which terminates the tail.

e. The anus, with a number of rugæ diverging from the contraction of that orifice.

Fig. 3. The head, in which is exhibited a front view of the three protuberances noticed in fig. 2.

a. a. Show the beginning of the body, or, perhaps, of the neck of the worm fore-shortened.

b. A front view of the mouth, as formed by the approximation of the three protuberances composing the head.

Fig. 4. Represents a portion of the tail in a front view, with the belly turned towards the spectator.

a. The anus with the rugæ noticed in fig. 2.

b. A front view of the hook-like extremity of the tail.

An Account of a Case of Small-Pox after Variolous Inoculation.

By BENJAMIN RUSH, *M. D. &c.*

ON the 25th of May, of the present year, I was called to visit captain Samuel Cassen, in consultation with Dr. Neill. He had been ill two days with an acute fever, attended with a bleeding from his nose. The doctor had bled him twice, and had purged him freely. We concurred in directing a third bleeding. The next day an eruption appeared all over his body, which I suspected to be the small-pox. He said that was impossible; for that I had inoculated him, twenty-six years before, for that disease, and, as a proof of it, showed me the mark usually left by the pustule on the arm; also two distinct pock-marks upon his leg. To prevent a mistake in this business I called upon his mother, who assured me, that he

had the usual fever and number of eruptions from inoculation; that he had been exposed to the disease, in her family, at the time his younger brother and sister were inoculated; and that she never had a doubt of his having had the genuine small-pox. The captain informed me further, that he had often met persons with the small-pox upon them, in the streets of Philadelphia, and that he had once visited a sailor who died with it in the East Indies, and had afterwards assisted in putting him into his coffin.

The pock, in the captain's case, were numerous and sore. They turned rather sooner than is common, but not more so than I had now and then seen them, before the general introduction of inoculation into our city.

The disease was taken from the captain's child, which had been inoculated. He sickened fourteen days after the pock began to turn in his child. One circumstance in the history of this case remains to be mentioned. Between the time the captain received the contagion from his child, and the day upon which he sickened, he had been exposed a whole day to a severe storm of rain on board his ship; in consequence of which, he had complained of having taken cold, several days before he was confined to his bed. Did this exposure to the storm of rain act as an exciting cause of the small-pox? and is it not possible that, under other circumstances, he might have escaped it? That the contagion of the small-pox enters the systems of those persons who have had it, in common with those who have not had it, is obvious from the symptoms of the variolous fever mentioned by Dr. Sydenham, which blended themselves with a fever of another kind, in persons who had had the small-pox in the natural way; also from the pock which appear in nurses who are much exposed to its contagion. I am not singular in

this opinion. It is admitted by Dr. Adams, in his sensible little work, entitled, " A Popular View of Vaccination, showing the Analogy between Small-Pox and Cow-Pox," in which he has ingeniously, and I believe correctly, explained several of the anomalies of the former disease.

But again: may not the fatigue to which nurses are exposed in attending patients in the natural small-pox, in addition to the large quantity of contagion imbibed by them, be the reason why they are more frequently affected with what are called " nurse-pocks," than physicians, and other more transient attendants upon such patients? Perhaps I should be safe in asserting, they are almost exclusively affected with them.

I submit these facts to the consideration of the enemies of vaccination, and the queries which follow them to the friends of free inquiry into medicine. From the benefit of the former, I shall subjoin an extract from a letter I received some years ago from Dr. Arnott, of York county, in this state.

" In your Treatise on Inoculation for the Small-Pox you allege, that there are certainly cases where there are the most irrefragable proofs of the infection implanted by inoculation being of a variolous nature, where the disorder has been afterwards taken in the natural way; in these cases, you suppose the variolous matter produced only a topical or cuticular disorder, and that a fever and eruption seem necessary for producing some impression on the whole system, in order to render it ever after incapable of receiving an impression of a similar nature. Of the truth of this I was fully persuaded till last summer, in the month of July, when (unhappily for me) some cases occurred, which I confess stagger me a good deal, and

have exposed me to a very great deal of country clamour. Permit me, sir, to lay the cases before you. About four years ago, I inoculated a young woman for the small-pox; on the third day the orifice seemed a little inflamed; the inflammation, however, went off till about the ninth day, when it began to rise up in the form of a pustule, at which time the girl sickened, and the fever continued four days, attended with the ordinary symptoms. About the evening of the fourth day, some pimples made their appearance here and there in different parts of the body, which remained until the seventh day; some of them filled with variolous matter, and did not disappear till the ninth or tenth day; they left marks behind them which are still visible. From these circumstances I was assured she had the small-pox, and confidently told her so. Since, about the 20th of March, 1798, the small-pox has been epidemical in this part of the country, and in July following she was called to nurse a family who had them of the confluent kind in the natural way. She had scratched a bit of skin off the little finger of her left hand, and being constantly employed in opening the pustules, moving the patients from one part to another, washing dirty clothes, &c. &c. it grew up in the form of a large pustule, and in the course of nine days she was attacked with the eruptive fever, which continued with considerable violence for four days, at which time the small-pox made their appearance, to the amount of five hundred, the most of which filled with good matter, and continued so till the ninth day, when they formed into scabs, and then disappeared.

"Another girl was attacked with the fever, which continued the usual length of time, attended with the common symptoms, after which, three pimples made their appearance, and then disappeared about the ninth day.

"Another girl, in the same house, seemed to have been infected by her; she was seized with a fever, which produced several pustules. The first of these girls being satisfied she had had the small-pox, exposed herself to the contagion, was infected a second time, and attacked with the confluent small-pox, of which she recovered with the greatest difficulty; upon the back of which, the other girl was attacked with the same disorder, which carried her off about the fourteenth day. A gentleman in this neighbourhood employed me to inoculate his children, seven in number; previous to which, he had got a servant girl from Baltimore, who had been inoculated before she came to the country. The mark of the orifice in her arm was perfectly visible, and she affirmed she had gone through all the different stages of the disorder; however, about the time the children were in a blackening state, she took the small-pox, but got over pretty easily.

"Such cases are certainly distressing both to families and practitioners: from these cases, however, which I know to be incontestible facts, I am really inclined to think, that a person is liable to be attacked with the small-pox more than once in his life."

On the Decomposition of Super-oxalate of Potash by Sub-nitrate of Mercury, or Red Precipitate of the Shops, and the Formation of Oxalated or Fulminating Mercury. *By* JAMES CUTBUSH.

MESSRS. AIKINS, in their "Chemical and Mineralogical Dictionary," having announced that Howard's fulminating mercury was an oxalate of that metal; and considering that the process of Mr. Howard, in the use of alcohol with a

solution of mercury, is not only extremely delicate, and affected by a variety of circumstances, so that the process would not uniformly succeed, but very expensive, I began accordingly a number of experiments, which were finally realized, and am able to give a more economical mode of preparation, having the same constituent parts as that prepared by Howard's process, which are oxygen, hydrogen, carbon, and mercury. All Messrs. Aikins say respecting it (in treating of the decomposition of alcohol) is the following: " Howard's fulminating mercury shows, in a striking manner, the generation of oxalic acid from alcohol, by the nitric acid, this singular compound being an oxalate of mercury, prepared by digesting alcohol with the nitrate of that metal." Therefore, learning from very respectable authorities that fulminating mercury is an oxalate, and considering the manner by which that combination takes place, which will appear hereafter, I began these experiments, modified in different ways, till success attended my endeavours. The present process may therefore be considered an original, cheap, and easy mode of preparation. Indeed, I had mentioned the circumstance to two scientific gentlemen, that, considering the composition of this singular substance, I thought the same preparation might be formed more economically. The recent information concerning the real composition of fulminating mercury, I presumed, was not *generally* known, and, accordingly, on the data above given, extracted from Aikin, I wrote an essay, and published it in Mr. Binns's paper, which is hereunto subjoined. The improvement in the present instance consists so far that *no alcohol is employed*, neither does occasion require, for the particular purpose, the dissolving of mercury in nitric acid, as both the articles, namely, salt of sorrel and red precipitate of the shops, may be procured of the druggists.

I took, in the experiment which succeeded, two parts of super-oxalate of potash (salt of sorrel), and one part of sub-nitrate of mercury (red precipitate), and mixed them intimately in a glass mortar. I then added sixteen parts of distilled water, and rubbed them intimately together; after which they were poured into a vial. On letting it stand for the space of ten minutes, I observed an apparent change, from a red to an ash colour; but this effect ensued more immediately on the surface; on shaking it, the decomposition became more general. At this time I collected a small portion of that which appeared to be the lightest, or rather of a more impalpable fineness, on a filtre, and washed it, which operation was continued till the fluid came off tasteless. I then dried it in the sun, and during this exposure it became of a dark brown, owing, I presume, to deoxidizement to a certain degree. Under the surface, however, of this crust it was of a lighter cast, and, on mixing it with a spatula, it became of a uniform ash colour. On trying a part of this powder on an anvil with a hammer, it exploded very violently, the comparison of which to that prepared by Howard's process was nearly equal. The remaining part of the vial was then introduced into a Florence flask, previously cleansed, and gradually heated over Argand's lamp; after which it was removed, and, when sufficiently cold, filtered: the powder thus obtained, being repeatedly washed and dried, amounted to one part, as nearly as I could conjecture. A part of the sub-nitrate, however, which did not appear to be decomposed, and which occupied the lowest place in the vessel, I considered would only injure the other, and was accordingly separated. When the precipitate thus obtained was taken off, and put on a tile and exposed to the sun's rays, it appeared of a darker colour than the other. A small quantity of this, treated in the same manner, gave a loud detonation.

We shall now consider the theory of this combination, and the principles of the decomposition of the sub-nitrate and super-oxalate, and conclude our essay with an extract from Howard's paper, and the observations of Bergmann on the decomposition of alcohol by nitric acid, and the formation of oxalic acid, with sundry remarks.

On mixing the sub-nitrate of mercury and super-oxalate of potash in a sufficient quantity of water, the latter is held in solution; but the first effect which ensues is the disengagement of the small proportion of nitric acid in the sub-nitrate, which, having a superior affinity for potash, quits its original combination, and combines with it, forming nitrate of potash or nitre, and, consequently, owing to its small proportion, separates a part of this alkali from its former combination with oxalic acid. The oxalic acid then disengaged, as well as that existing in superabundance constituting the super-oxalate, in the next place unites with the oxyd of mercury, and forms the oxalate of this metal in the form of a powder. The compound, thus formed, collected on a filtre, and thoroughly washed to free it of nitrate of potash, and any portion of undecomposed oxalate of potash, when dried, formed the fulminating mercury in question. The small quantity of undecomposed red precipitate, I attribute to the proportion of super-oxalate not being sufficient for that purpose; but, the proportion being small, I considered it unnecessary to repeat the experiment. In producing the detonation, I remarked that the second, third, and fourth strokes on different portions of powder succeeded much better than the first, and also by heating the hammer. This circumstance would, therefore, seem to be owing to the incitement of temperature in producing the decomposition with more facility. This powder is said to take fire at 368° Fahrenheit: hence, from

the above circumstance, it is not an uncommon thing with our blacksmiths, on some occasions, to light the coal of their forges by this means.

I have no doubt that nitrate of mercury, decomposed by super-oxalate of potash, or the neutral oxalate, would give a precipitate, which, on the same principle, when *well* washed and dried, would fulminate*. This mode, indeed, would be less economical than the one described. I performed a number of experiments, some time since, to produce different fulminating powders with the oxyds of metals and hyper-oxymuriate of potash, &c. which I shall publish at some future period.

In one of the late European Magazines, I find a specification of a patent for forming a lack, to be used expressly for the purpose of detonating fulminating mercury. If this should ever be adopted, the introduction of the present process, whereby the fulminating mercury may be made far cheaper, cannot fail being of importance. By obtaining oxalic from sugar, &c. with nitric acid, in consequence of repeated distillation, causing the acidification; this, combined with the oxyde of mercury, would also afford the oxalate of this metal.

* I have tried this mode, but my experiment did not succeed, owing entirely, I presume, to the precipitate not being sufficiently washed. It is evident, that when oxalate of potash, and nitrate of mercury, when in solution, are mixed together, a double decomposition will take place; the nitric acid passes to the potash, forming nitrate of potash, whilst the oxalic acid unites with the oxyde of mercury; thus precipitated, forming an oxalate of this metal. *Query*: Would not the arsenious acid, digested with a solution of super-oxalate of potash, afford a fulminating arsenic similar to to that of Praust's? and would not the *purple powder of cassius* (a preparation of gold), treated in the same manner, give fulminating gold, &c.? It is my intention to make some experiments on these subjects at a future period.

To prepare Mr. Howard's fulminating mercury, 100 grains (or a greater proportional quantity, not exceeding 500) of mercury are to be dissolved with heat, in a measured ounce and a half of nitric acid. The solution being poured cold upon two measured ounces of alcohol, previously introduced into any convenient glass vessel, a moderate heat is to be applied till effervescence is excited. A white fume then begins to undulate on the surface of the liquor, and the powder will be gradually precipitated on the cessation of action and re-action. The precipitate is to be immediately collected on a filtre, well washed with distilled water, and cautiously dried in a heat not exceeding that of a water bath. The *immediate* washing of the powder is material, because it is liable to the re-action of the nitric acid: it is very subject to the action of light. One hundred grains of mercury will give about 120 or 130 of the powder. See Philosophical Transactions, 1800, p. 214.

Mr. Accum, in Nicholson's Journal, vol. 1, p. 299, notices a singular circumstance respecting this preparation: that, on exposing four ounces of it, still wet, on a chalk stone, for three months, he found it converted into a brilliant black powder, and, on introducing it into a vial, the mercury became metallized, with an emission of heat.

Respecting the decomposition of alcohol, and the production of oxalic acid, in consequence of the action of nitric acid on this substance, we have some experiments of professor Bergmann, and others; and accordingly the same principles may be applied to explain the production of fulminating mercury by Howard's formulæ, which compound is proved from this and other sources to be an oxalate of this metal.

Scheele remarked, that, in the action of sulphuric acid on alcohol, acetous acid was produced, the radical being the same as the oxalic; but, when alcohol is treated with nitric acid, as it acts with more energy in the decomposition of alcohol, oxalic acid is formed. Bergmann, in consequence, mentions that the residue left after distilling twenty-four parts of nitrous acid, and eight of alcohol, if re-dissolved and chrystalized, gives three parts of chrystals of oxalic acid. Messrs. Aikins, in their "CHEMICAL AND MINERALOGICAL DICTIONARY," a very valuable work, say that the decomposition of alcohol, in the production of oxalic acid, may be shown by a simple experiment, as the following:

On one pound of strong alcohol, put into a long-necked retort, and connected with the pneumatic apparatus, add four ounces of strong nitrous acid, with great caution, at repeated times, and apply a very gentle heat; nitrous æther, nitric oxyde gas (nitrous gas), and carbonic acid are first given out, which last may be absorbed by lime-water, and rendered sensible. When no more gas is given out, add three ounces more of the nitrous acid to the residue, which, again distilled, give the same products as at first. Then, by slow evaporation of the liquor left in the retort, crystals of oxalic acid will be obtained. On the other hand, by too great an addition of nitrous acid, or the boiling of the materials brisk, much acetous acid (the same as distilled vinegar) is also obtained. This circumstance is sufficient to prove, as the same phenomena are apparent in preparing fulminating mercury by Howard's process, that oxalic acid is generated during the action of nitric acid, and this takes place in consequence of the acid undergoing a decomposition, whereby the oxygen acidifies the alcohol (carbon and hydrogen) to a certain degree, and produces the oxalic acid with proper proportions, while other combinations, as nitric oxyde,

nitrous æther, &c. are generated. It appears, also, in preparing Howard's fulminating mercury, that as soon as oxalic acid is formed, the alcohol decomposes the nitrate of mercury, and unites with the metal, which is oxydized, in a precipitate; but this precipitate is liable to be acted upon, and consequently decomposed, by the nitric acid; it would therefore seem that nitrate of mercury was again formed, and the oxalic acid disengaged; by immediate washing, however, this effect does not ensue. Those vegetable substances, containing the radical or base of oxalic acid, as starch, sugar, &c. when treated with nitric acid become acidified, and afford, of course, oxalic acid, and this acidification originates by the decomposition of nitric acid, nitric oxyde gas being evolved, whilst the quantity of oxygen, not combined with azate in the nitric oxyde gas, goes to the oxygenizement of the oxalic bases. It is therefore obvious, that, if oxalic acid, formed by any of these methods, is combined with oxyde of mercury, the oxalate will be obtained. There is one circumstance to be observed, that if sub-nitrate of mercury is digested in a solution of super-oxalate of potash, the indication of the formation of fulminating mercury will *uniformly* be, when the *red* colour is destroyed and the *grey* appears; and this may be a criterion from whence we may judge of our success.

The annexed note on Howard's fulminating mercury, above alluded to, which appeared the 17th of August, explains sufficiently the principles of the formation of this substance by Howard's process, and the detonation occasioned by concussion and other means*.

* HOWARD'S FULMINATING MERCURY.

Since Mr. Howard made known to the scientific world the discovery of fulminating mercury, various theories have been advanced respecting the nature

Observations on Cynanche Trachealis; with a new Theory of that Complaint, &c. By JOHN STEARNS, *M. D. of Waterford, Saratoga County (N. Y.)*

Waterford, Saratoga County (N. Y.), July 30th, 1808.

DEAR SIR,

I HAVE observed several communications in the Medical Museum, on the subject of *cynanche trachealis*. None of the methods of cure therein detailed perfectly correspond with the one I have adopted, and which, in the course of several years' practice, I have found to be uniformly successful. The

and composition of this singular substance. Some have supposed that it is hydro-guretted oxyde of mercury; others, a combination of nitric æther and oxyde; but the explanation of the process, the *rationale* of the combination, and the detonation, have been satisfactorily explained, in consequence of experiment, upon a principle we cannot doubt.

It is proved to be a compound of oxyde of mercury and oxalic acid, forming oxalate of mercury. From the subsequent mode of preparation, the theory of this combination will appear obvious. On making a solution of mercury in nitric acid in proper proportion, and adding a sufficient quantity of alcohol, a decomposition, and of course a reciprocal change of some of the elements ensue. The hydrogen and carbon, the constituent parts of alcohol, a part of which is necessary for the generation of fulminating mercury, combine with a given quantity of oxygen, and consequently generates the oxalic acid, the proportions being adequate to this combination, and the mercury separated at this instant unites with a portion of oxygen from the same acid into an oxyde, which, combining with the oxalic acid thus formed in a precipitate, produces the oxalate of mercury.

Another combination at the same time, as the operator will readily perceive, takes place, which is the formation of æther in a disengaged state, and ensues, as it is said, by an oxygenizement, in a given degree of the remaining alcohol.

following remarks and treatment of this disorder, I therefore submit to your disposal.

The croup has a frequent occurrence in this vicinity. I have fifty cases on record during the three years immediately preceding. Two of these cases proved fatal; one of which was unable to retain the medicine, but by a constant vomiting ejected every thing from its stomach; the other had a pulmonic affection from its birth, and expired a few hours after I first saw it. I can, therefore, safely assert, that this mode of treatment has infallibly succeeded in every case of *simple cynanche trachealis*, in which the medicines have been long enough retained to produce their customary effects. This disorder has, therefore, become completely divested of its terrors, and yields to medicine with as much facility as the most simple ailment of the human frame.

It therefore appears that one part of the alcohol (which unquestionably is the precipitant) is converted into æther, while another portion, containing, of course, the radical of oxalic acid (hydrogen and carbon), unites with a given proportion of oxygen, and that combination again with the oxyde of mercury produced at the same time. The nitric acid, therefore, is decomposed, and in the above manner; it remains then to show the *rationale* of that detonation. From the principles of the combination, and the consideration of the laws of affinity, it appears as follows: the temperature being raised to a sufficient degree, disposes the elements to arrange themselves in a new order; this temperature is excited by the blow of a hammer or other causes. The hydrogen unites with the oxygen of the oxyde, and forms water; the carbon then, combining with another portion of oxygen belonging to the oxalic acid, forms the carbonic acid; *at this instant the mercury is volatilized*, being reduced, and this change ensues momently, in consequence of which the detonation is produced.

According to a former theory, it was merely supposed that water was alone formed, in consequence of admitting that this fulminating substance was only a hydro-guretted oxyde of mercury. C.

Contrary to the opinion of most medical theorists, I am induced to believe that this disorder is not primarily an inflammatory affection of the trachea. I am warranted in this opinion not only by the prominent symptoms, but by the appearances on dissection. That inflammation does sometimes take place, I readily concede; but this is not necessarily connected with the disorder, and, when it does succeed, is always the effect of secondary causes. Cold, applied to the surface of the body generally, or to the trachea in particular, is, perhaps, the invariable cause of this complaint. Its operation is to induce a torpor of the absorbents of the trachea, either immediately by a direct application to their mouths, or by sympathy when applied to remote parts of the body. Thus paralized, they cease to absorb the mucus exuded by the exhalants. This mucus, consequently, accumulates, till, indurated by evaporation, it is converted into a tough membrane that completely obstructs respiration. Does this torpid state of the absorbents necessarily induce local inflammation? I think not; for although the action of the exhalants of the trachea may be partially increased from the irritation of the indurated mucus, yet there are no indications of that degree of inflammation that requires depletion, and it appears to me that every remedy that has no other operation but to induce direct debility, must increase the torpidity of the absorbents, and consequently expedite the fatal termination of the disease. Hence bleeding is, in my opinion, a very hazardous remedy, and ought never to be prescribed in simple cases of croup.

The indications of cure are simply to restore the healthy action of the absorbents of the trachea. Whenever this object is accomplished, the morbid symptoms vanish; the membrane is partially absorbed, while its detached parts are expectorated.

and the patient becomes convalescent. Violent emetics have sometimes been prescribed with success, and, whenever they do succeed, it is unquestionably owing to the action of the absorbents being excited by the convulsive efforts which they induce. But, when they fail of exciting the absorbents of the trachea, the debility which they induce is accompanied by an exacerbation of all the symptoms; I have, therefore, long since, laid aside bleeding and violent emetics in the cure of this disorder, and place my whole dependence on calomel, cerated glass of antimony, and seneka. The two first medicines I always give combined in very large doses, and repeat them every six or eight hours, till they complete the cure. They have always succeeded with me when used without the seneka, but, since this medicine has been so highly extolled by Dr. Archer, I have generally accelerated the cure by administering it in the intervals, agreeably to his directions.

Twenty grains of calomel, combined with eight grains of cerated glass of antimony, is the dose that I generally prescribe to an infant of a year old, when the disease has assumed its most alarming symptoms. This dose will operate two or three times as an emetic, and as often by stool, and will always alleviate the complaint, and sometimes effect a cure. If the disorder continues after the operation of this dose, I give the decoction of seneka, and, at the expiration of every eight hours, repeat the dose till the cure is complete. In common cases one dose is sufficient, and I have never found it necessary to give more than four.

I am aware of the censure I may incur from the empiric boldness of this prescription. My only answer is, that the unparalleled success of this mode of treatment, for ten years of

extensive practice, amply justifies its continuance, and, I have no doubt, will remove every objection in the minds of its opponents, if they will give it a fair experiment.

JOHN STEARNS.

Dr. J. R. Coxe.

An Account of the Efficacy of Mercury in Pulmonary Consumption, and of a new Mode of conveying that Medicine to the Lungs. In a letter from Dr. P. W. LITTLE *to Dr.* RUSH.

Mercersburg (V.), February 18th, 1808.

DEAR SIR,

IN your lectures you very justly observe, "diseases are incurable only from their being considered so." I feel happy I have it in my power to add several proofs to those already recorded in favour of your maxim, and in contradiction to the prevailing prejudice, having within the last year cured four cases of confirmed consumption. My principal medicine in counteracting the morbid action in this volcanic disease, after the inflammatory diathesis is sufficiently subdued by venesection, cathartics, blisters, and the antimonial powders, are mercury exhibited in the *form of gas*, and *inhaled* into the lungs, digitalis sac. saturni, and the usual tonics. I prefer the above mode of exhibiting mercury for the following reasons:

1st. It has a greater surface to act upon. "Dr. Kiel supposes that the lungs, from the peculiar structure of the brancheal vessels and air vesicles, expose a surface to the action of the air, equal to the extent of the whole external and visible surface of the body."

2d. It acts *specifically* upon the part diseased, thereby promoting absorption and discussion of the tubercles. It has been observed by some one, but by whom I do not now recollect, that "we know of no medicine that can exert its specific effects upon the lungs, by dispersing tuberculous concretions; nor is it probable, from what we know of the animal economy, that such will ever be discovered." From our knowledge of the effects of mercury upon the system, and in discussing venereal buboes, chancres, &c. I flatter myself that, by the above mode of exhibiting it, we are in possession of that medicine which was supposed never would be "discovered;" and, should it be deemed necessary, the mercury may be conveyed into the lungs in the following manner: a hollow bougie, of the elastic gum, with a bent probe like a catheter, may be introduced by the nostril, and passed into the larynx, which may be known by the flame of a candle being agitated when presented to the orifice. The probe may then be withdrawn with one hand, whilst the bougie is supported and fixed by the other. Being thus introduced, it may then be fixed to a vial containing the mercurial gas, or to Mudge's inhaler.

3d. It produces a ptyalism much more certainly and speedily, without that tendency which mercury usually has to run off by the bowels, which unhappy tendency appears to be the greatest barrier to the cure of phthisis pulmonalis; for, in almost every instance in which a ptyalism has been excited, a cure has been performed.

Miss P. M'C—— (a phthisical patient) was salivated in twenty-four hours, by inhaling the mercurial gas frequently in that time; Mr. S—— was also salivated by the mercurial gas in twenty hours; Miss J. M'C——, was salivated in forty-eight hours; and Miss B. M'K——, in thirty, by the same medi-

cine. Miss I. M'C—— had a violent diarrhœa, and was evidently in the last stage of phthisis. Miss R. S——, with "galloping consumption," began to complain, in twelve hours, of her mouth being sore, after she began with the mercurial gas; Miss S—— supposed the discharge to be at least a pint of matter in twenty-four hours. She was reduced to the lowest extremity; had a diarrhœa and swelled legs, but happily recovered.

You have observed in your Inquiries, when speaking of the "second, or hectic state of consumption," that the removal of the fever was the greatest desideratum in the cure of consumption. In the above cases, I derived the most happy effects from the use of digitalis and sac. saturn. with opium at night. In some instances, it has acted like a charm in alleviating all the hectic symptoms. I have found no medicine equal to the mercurial water in alleviating the cough, when inhaled. As experience, reason, and free inquiry are the only effectual agents against error, your opinion upon the above method of treating, or rather of exhibiting mercury in phthisis pulmonalis will be anxiously and cheerfully received. May the benedictions of Heaven attend you, and may God long preserve your useful life*!

P. W. Little.

Dr. Benjamin Rush.

Experiments to Ascertain the Purity of the Air in different Situations. *By* Adam Seybert, *M. D. &c.*

THE results of many experiments, to ascertain the purity of the air, which I made on a passage from Bordeaux to

* Dr. Little has omitted informing us how to prepare the mercurial gas for use. Editor.

Philadelphia, in 1796, are published in the fourth volume of the American Philosophical Transactions. These experiments were performed under various circumstances, though always with the same test, viz. nitrous air, obtained by means of brass filings and diluted nitric acid.

From the trials just alluded to, it was inferred that the *oxygenation* of our atmosphere was the *same* in every situation *over land*, whether in large cities or in the country; at the lowest and at considerable heights; at the same time I conceived myself warranted in making the following conclusion, "that the air over a large body of water is always purer, *cæteris paribus*, than that of the adjoining land."

That the air over the ocean is purer (more highly oxygenated) than that over land, is a very popular opinion, and might be maintained, by reasoning, to an unlimited extent. With some hesitation I doubted its correctness, but could not abandon the idea until the experiments of many European philosophers were published. We now maintain, from a number of concurring experiments which were made in Egypt, and in every part of Europe, by men of the first eminence, that neither climate, nor temperature, nor distance above the surface of the earth, produce the smallest variation in the relative quantities of oxygen and azote, as constituents of our common atmosphere. Humboldt and Gay Lussac have proved that the atmosphere of theatres, which had been breathed for some time by a crowded assembly, differed only one per cent. as to the proportion of oxygen which it contained, from the atmospheric air on the outside of the building; and Seguin informs us, that he examined the air of hospital wards, after they had been closed for twelve hours, which exhibited "an insupportably infectious smell," and found it almost as pure as atmospheric air.

Subsequent experiments have proved, that trials by the nitrous test are very fallacious, and that no certain inferences can be drawn from them. Other, and more certain means, having been proposed and employed by chemical philosophers in Europe, I was desirous to ascertain what the result would be in America, when the same methods were made use of. With this view, the experiments related in the annexed table were made.

The test employed on these occasions was a solution of sulphuret of potash in water. The liquid was, for some time, exposed in contact with atmospheric air, before it was bottled for use, thereby to prevent any absorption of azote in the experiments.

Cavendish, Marti, Berthollet, Fourcroy, and Davy, from numerous trials which they made, determine the proportion of oxygen gas in atmospheric air, to be between twenty and twenty-three hundredths. It will be found, that the results which we obtained, cannot be said, in a general way, to differ from those just mentioned. I never found the proportion of oxygen so great as $\frac{23}{100}$, and commonly between $\frac{20}{100}$ and $\frac{22}{100}$. These trifling differences may be attributed to variations which are sensible to the thermometer, barometer, and to circumstances in the mode of operating. Although the results are stated in round numbers at $\frac{20}{100}$, I do not recollect an instance where the absorption was not a fraction above that number.

I now consider as erroneous, the opinion formerly adopted by me, that the air over large bodies of water is purer than that over the adjoining land. This is very evident from the results of all the trials, but more especially from those had November 23d, 1802.

Date of the experiment.	Temperature.	State of the atmosphere.	The air which was tested.	Quantity absorbed by the liquid sulphuret of potash.	Effects of lime water.
1802.					
August 14th*.	77°	Wind S. W. partially cloudy.	In the yard back of No. 114, Market-street, where it was healthy	20.0	No experiment.
—	—	—	In the middle of Market-street, opposite to Mr. Dunlap's. This might be looked upon as a country situation	20.0	do.
—	—	—	Directly opposite to Mr Innes's brewery, Water near Vine-street. Several persons died in Mr. Innes's house of the prevailing fever	21.0	do.
—	—	—	In Front near Vine-street, where the fever was prevalent	21.0	do.
20th.	76	Wind S. W. cloudy.	The experiments related the 14th inst. were repeated this day	in every case 20.0	Did not render lime wat. turbid.
November 4th.	not ascertained	Wind S. W.	From the neighbourhood where the yellow fever had prevailed	20.0	do.
—	do.	—	From the Neck, a low situation; many marshes in the neighbourhood	20.0	do.†
—	do.	—	Opposite Mr. Dunlap's	20.0	do.
—	do.	—	Air of a room in which a person continued ill of the late prevailing fever. A wood fire was kept in the apartment	20.0	Rendered lime water turbid‡.

* Yellow fever prevailing in and about North Water and Front, near Vine-street.

† Air collected at different times, during the summer of 1798, in various situations in the Neck, when agitated with lime water, caused a white precipitate, more or less copious. The lime water, in the experiment above stated, remained unaltered. I attribute this circumstance, in a great degree, to the frost having prevented the formation of carbonic acid gas.

‡ The occurrence of carbonic acid gas in this case may be attributed to the wood fire in the apartment.

Date of the experiment.	Temperature.	State of the atmosphere.	The air which was tested.	Quantity absorbed by the liquid sulphuret of potash.	Effects of lime water.
1802.					
November 23d.	not ascertained	Wind S. W. sky clear.	Half way between the shore and the sand bar opposite to Philadelphia	22.0	No experiment.
——	do.	——	Half way between the sand bar and the Jersey shore	22.0	do.
——	do.	——	Quarter of a mile from the shore of the Delaware river, on the Jersey side, near a flourishing wood	22.0	do.
——	do.	——	In the yard back of No. 114, Market-street	22.0	do.
1808.					
July 22d, ¼ past 2 o'clock, P. M.	82°	Wind S. W. partially cloudy.	Opposite Catherine-street, on board a packet-boat	20.0	Rendered lime water turbid*.
½ past 6 o'clock, P. M.	76	do.	Opposite town of Chester	20.0	No experiment.
23d, ½ past 6, A. M.	74	Wd. S. S W. cloudy	Opposite Reedy Island	20.0	Lime wat. unaltr.
¼ past 6, P. M.	78	Wind S. E partially cloudy.	Opposite Bombay Hook. Shore distant 3 miles	20.0	do.
24th, 6 o'clock, A. M.	79	Wind S. cloudy.	Cape May distant 8 miles	20.0	do.
10 o'clock, A. M.	80	Wind S. E. sky clear.	On the beach of Cape May, western shore	20.0	do.
25th, 10 o'clock, A. M.	82	Wind S. E. sky clear.	On the beach of Cape Island, eastern shore of Cape May	20.0	do.
August 1st.	78	Wind W. N. W. sky clear	Delaware Bay, Cape Henlopen light-house bearing S. W. distant 22 miles	20.0	do.

* There were many persons on board the boat, and a fire was kept for the purpose of cooking. The passengers were anxious to witness the experiment, and crowded about me. Could the presence of carbonic acid gas in this trial be owing to the respiration of the people?

Account of the Discovery of the Vaccine Disease among the Cows, in Lancaster County, Pennsylvania. In two letters from Mr. JOHN EBERLE, *student of medicine, to Dr.* KLAPP.

Southwark, July, 1808.

SIR,

IF it should meet your approbation, the following correspondence may occupy a place in one of the future numbers of the Medical Museum.

The intelligence of a recent discovery of the vaccine disease among the cows, in Lancaster county, Pennsylvania, cannot be uninteresting to your readers.

I am, sir, yours respectfully,

JOSEPH KLAPP.

Dr. J. R. Coxe.

Extract of a Letter, dated Lancaster, July 4th, 1808, *from Mr.* JOHN EBERLE, *student of medicine, to* JOSEPH KLAPP, *M. D.*

DEAR SIR,

I HAVE met with a circumstance which will perhaps be interesting to you. It is this: about a month ago, I was asked by a country woman why the vaccine disease was called cow-pox? I told her it originally came from the cows, and gave her some description of it. She then informed me that one of her cows had the disease, such as I described, and that they could not milk her. I went and examined her, took a little of the virus on the point of a lancet, with which I vaccinated a young man, who never had either the small-pox or cow-pox.

On the third day, the place of puncture was a little inflamed and elevated, exactly like the common cow-pock at that period. The local affection afterwards went on with the most coincident regularity of the genuine disease. The areola was beautiful, the pock of the usual form and appearance, and the sympathetic fever considerable. In short, I am convinced that it was the genuine cow-pox. I have enclosed the scab for you*.

A Reply to Mr. John Eberle.

Southwark, July 10th, 1808.

DEAR SIR,

I received your letter, per the mail, this morning, and as I feel much interested about the discovery which you suppose you have made of the vaccine disease among the cows of Lancaster county, I have immediately written a few lines in reply, to solicit further information.

Hitherto I believe this curious disease has not been noticed among the cows of this country, except in one or two instances. Perhaps, to ascertain that it prevails almost as common with our American cows, as it does with those of the Shire counties of England, it is only necessary to make a general and strict inquiry. Herewith I send some active variolous matter, with which I wish you to test the liability of the young man whom you vaccinated with the fresh matter from the cow, to the morbid influence of the small-pox. When the experiment has been made, please to acquaint with its result

Your humble servant,

JOSEPH KLAPP.

* This scab was given to the editor by Dr. K. It was most unquestionably the scab of the genuine vaccine. It was tried in one case ineffectually.

EDITOR.

To this letter Mr. Eberle wrote the following very satisfactory answer.

DEAR SIR, *Lancaster, July 18th*, 1808.

I received yours of the 12th inst. and at your desire immediately inoculated the man who had been previously vaccinated with the matter from the cow, with the variolous matter you sent me. The infection did not take; the place of puncture was slightly raised on the third day, but has since totally disappeared. This is, surely, an incontrovertible evidence of the genuineness of the vaccine matter with which he had been vaccinated. Perhaps, indeed, as you suppose, the disease is much more common among the cows of this country than is generally imagined. I have since heard many say, that they often saw pustules on the udders of cows, which it is very presumable were of the cow-pox kind. There is an experiment I should like to make (perhaps it has been made), which, if rightly conducted, would probably throw some light on the nature of the vaccine disease: to inoculate a cow with variolous matter, then a human subject with the newly formed matter in the pustules of the cow. The disease might be much milder, and, perhaps, by successive repetitions of inoculation with the same matter on cows, the mild form of the small-pox, or the cow-pox, might be generated from the most virulent variolous infection. I really believe your theory* of the cow-pox is correct, and I am determined to put it to the test of experiment. I am yours, &c.

JOHN EBERLE.

Dr. Klapp.

* The theory alluded to, is a supposition that the vaccine disease and small-pox are modifications of one and the same disease. It was advanced about two years since, on the evidence of ancient history, and reasoning on the apparent identity of the diseases.